LETTERS TO THE EDITOR.

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Breath Figures.

ON reading Lord Rayleigh's short article on breath figures in NATURE of May 25 and finding he was not satisfied with the explanation I offered of them, I thought it would be as well to see if it was possible to get some further information on the subject. For this purpose I repeated the experiments with variations in the conditions. As the writer of the article seemed to think that the hot gases act in some way in cleaning the surface of the plate, the experiments were now made at lower temperatures. In place of the blow-pipe flame a Bunsen burner was used, and in order to protect part of the surface from the action of the hot gases, the plate was supported on two thick iron bars, so as to confine the action of the gases to the narrow space between them, and further, the glass plate was kept as cold as possible. It was intended to use ice for this purpose, but it was found that it kept the under surface always dewed, so that the gases could not come into contact with the plate. Wet blotting-paper placed on the upper surface was found to keep the plate cool enough, and yet not so cold as to cause the deposition of dew. It was found that it was not necessary to pass the plate through the flame to get the breath figures; it could be held some distance above it with a similar result, but, as might be expected, a longer time was necessary than when in the flame, not only on account of the lower temperature of the gases, but also on account of the plate getting heated by the longer exposure.

In these experiments it was noticed that it is not enough to allow the hot gases to travel along the space between the two bars, as the hot gases only produce the effect where they first strike the surface of the plate, and if it is dust that is the cause of the action, it will be deposited where the gases first touch the cold surface, the onward flowing gases near the plate being nearly free from dust, as well as being at a lower temperature. The plate should therefore be moved over the stream of hot gases in such a way that fresh gases come in contact with the different parts of the exposed surface. The plate should also be held at a considerable angle to allow of a free flow of the gases over it.

If the action of the hot gases is a cleansing one, then heat alone ought to produce the same effect as the flame, and cause the glass to take an even film of dew. To test this a cleaned plate was placed—clean side up—on a metal plate somewhat larger than itself, and the metal plate highly heated with a Bunsen burner. Some plates were heated slightly, others to a temperature far higher than those acted on directly by the flame, yet when cold these plates were unchanged. Part of the plate was freshly cleaned, when it was found that the deposit of moisture was the same all over the plate. Heat alone evidently has no effect. I may here mention that those breath figures are best developed by rubbing the back of the plate with a piece of ice. By this process the image remains longer in view, and gives time for inspection.

An examination, by means of a short-focussed lens and proper illumination, of the moisture condensed on the surface of a glass plate, reveals some interesting points. When the dew first begins to appear the water is seen to be deposited in the form of very small lenses nearly touching each other. On looking through the plate while the dew is still condensing, nothing can be seen through it. It acts very much like a piece of ordinary ground glass, a landscape being invisible; even the sky-line does not show. If the cooling be now stopped and the deposit allowed to evaporate it will be noticed that the spaces between the lenses widen, the lenses drawing in their edges and leaving clear glass. Objects can now be seen through the plate, the transparency increasing as the dew evaporates. If, however, in place of stopping the condensation at its early stages we keep the plate cooled so as to cause more moisture to be deposited, the lenses will be seen to grow into each other and coalesce, until at last the regular deposit of little lenses grows into irregularly shaped blotches of water of considerable size. If the condensation be stopped at this stage and the plate dried and again cooled, it will be found that the appearance of the condensation is not the same as it was the first time. The surface of the plate has been changed by the first condensation. On the dew making its appearance the second time the uniformity in the lenses is gone. In addition to a general ground of small lenses there now appears a number of much larger ones, and as these are distributed over the plate at about the same distance from each other as that occupied by the large evaporated blotches of water, they would appear to be produced by something left by the water on the glass. By observing a particular group of blotches near a mark on the plate, it was noted that each one of these reappeared on each successive dewing. The something which is left after evaporation has evidently a considerable affinity for water, as these large lenses condense more water than an equal area of small ones. They stand high, and are the last to evaporate.

But perhaps it will be asked, How can anything be left after the evaporation of pure distilled water? At first sight one might feel inclined to say that nothing could be left. It is, however, evident that something has been left—probably dust and gaseous impurities condensed along with the water—and I may point out that something similar happens when cloud particles of water are formed on ions in the presence of certain gases. In this case there are no solid nuclei, yet the drops do not thoroughly evaporate, but leave behind them something large enough to be nuclei of condensation in air only very slightly above saturation. These observations on the behaviour of glass plates show how delicate their surfaces are and how easily the condensation on them may be altered.

The something which produces these breath figures formed by flames is of such a nature that it acts more powerfully the higher the temperature of the gases, that is, the greater the difference in temperature is between the gases and the glass. Of course, it is possible that the effect may be due to some gas or gases condensed on the glass, and these gases having an affinity for water. On the other hand, it may be due to the gases depositing their fine dust; and while what we call clean glass repels water, dust, on the contrary, attracts it. The fact that this form of breath figure is easily washed off the plate seems to point rather to dust than absorbed gases as the cause.

The formation of these breath figures does not appear to be so much a question of cleanliness as of the nature of the foreign matter on the surface of the glass. If the impurity is of a water-repelling nature we get the lenticular deposit; if it has an affinity for water we get the uniform film. For instance, if the plate has been previously touched with paraffin oil, it will, even after a good deal of cleaning, give a very white obscure deposit, due to the great convexity of the lenses. On the other hand, if the surface has been previously treated with caustic soda, the lenses are flat and the plate more transparent.

As these experiments have generally been made with gas flames, and as the flame is an important factor, it was thought advisable to try other flames. Hydrogen when burned in filtered air, I have shown, gives rise to no fine dust, and, so far as I know, this is the only form of combustion which does not. It therefore ought to have been tested, but as the apparatus required would be somewhat complicated, it has not been done. Alcohol, however, was tried; with it I could only succeed in getting very slight indications of any action, though the plate was heated far more by it than what gave a marked effect with the Bunsen flame. This result is what we might expect if the figures are due to dust. A sulphur flame was also tried, and, as might be expected, gave very marked results with a very small flame. This might be given as a typical case of the effect of condensed vapour. Something of this kind may play a small part in the figures produced by gas flames.

It was thought it might be interesting to see how the fine dust on our windows acted towards condensed water. The fine dust in the air is deposited on our windows in the

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same way as I imagine the dust of the flame to be deposited on the plates, namely, by difference of temperature, our rooms being warm and windows cold at night. The only difference is in the rate of deposit, due to there being less dust in the room air and to the smaller difference in temperature in the case of the window. Cleaning a small part of a window which had been clean ten days previously, a piece of ice was rubbed over the outer surface until dew began to deposit on the inside. The deposit on the ten-day-old surface was different from the newly cleaned one, but not greatly so. While the newly cleaned part was covered with the usual little lens-like discs, the older surface was covered with much larger and irregularly shaped blotches of film. As the nights had not been cold since the window was cleaned, we can hardly expect much dust to have collected in ten days; so another window was tried which had not been cleaned for some months. Repeating the cleaning and cooling on this window, it was found that while the newly cleaned part carried a ground-glass-like deposit, the uncleaned part was sufficiently clear for the landscape to be seen through it. These tests show that dust on glass does tend to cause water condensed on its surface to spread and take the film form in the same way as glass that has been exposed to flame or to hot gases.

Coming, now, to Quincke's experiment, referred to by Lord Rayleigh, in which sulphuric acid is shown to produce the same effect as the blow-pipe flame, this and the experiment with hydrofluoric acid seem to have inclined Lord Rayleigh to think that cleanliness was the cause of the breath figures. But does either sulphuric acid or hydrofluoric acid prove cleanliness? I have doubts. I know I am on dangerous ground in differing from Lord Rayleigh on anything connected with surface action, yet I have recently had my lesson on how some substances cling to glass in spite of efforts to get rid of them, and I think it is not improbable that some residual of both sulphuric and hydrofluoric acid may cling to the glass in spite of washing. Recently I was making an investigation in which was required a little iodine vapour, and for this purpose put a small crystal of iodine in a flask from which the vapour was drawn as required. Afterwards the investigation took another turn, and the flask was used for other purposes, but many days' work were lost owing to that flask. Results were obtained with it which were contrary to previous experience. As suspicion centred on the flask it was discarded, and not until a new flask had replaced it could satisfactory work be done. Yet all this loss of time was occasioned by a residual quantity of iodine, which the washings with alcohol, acids, soap water and a sponge, had not succeeded in removing. After that experience I confess to being sceptical of absolute cleanliness of glass after being touched with sulphuric or hydrofluoric acid. Any residual of these substances, as they have an affinity for water, would tend to form films and not little lens-like patches. Though breath figures may be formed by dust, yet there are evidently other ways of altering the surface of the glass and causing it to repel or attract water, and so making the surface capable of giving breath figures.

John Aitken.

Ardenlea, Falkirk, June 5.

The "Vernal Phytoplankton Maximum."

In Nature for April 27 it was stated, in connection with the plankton statistics taken periodically in the Irish Sea from the Port Erin Biological Station, that (p. 289) "the outstanding fact in this season's work, so far, is that the diatoms are unusually scarce and late. The vernal phytoplankton maximum has not yet arrived."

That statement referred to the collections up to the middle of April. During the rest of April the catches remained small—for the most part 1, 2, or 3 cubic centimetres in a standard haul of the fine silk net. In May the approximate quantities (they have not yet been accurately measured), in the same net, run as follows:—

	c.c.
May 1	2
,, 4	1
,, 10	10
,, 13	40
,, 16	60
,, 19	50
May 22	35
,, 25	10
,, 29	15
June 1	15
,, 3	60
,, 5	50

The large catches on May 13–22 were mainly composed of Chætoceras (*C. debile*, and a few other species of diatoms), while the sudden increase in the beginning of June is due almost wholly to Rhizosolenia (mainly *R. semispina*).

Last year we found that the vernal phytoplankton appeared as two well-marked maxima, one in April, caused by species of Chætoceras and Lauderia, and a second in June, composed of Rhizosolenia and Guinardia.

It is evident that the "vernal maximum" is really a complex made up of the maxima of several different species or groups of species which seem to occur in a definite sequence, but may be earlier or later, more spread out in one year or more telescoped together to form a single diatom maximum in another. Moreover, the individual species or groups of species may be more abundant one year than another.

In the present year, if we look at the three genera that usually bulk largest in our spring and early summer collections, we find that Biddulphia reached its climax in March (but continued throughout most of April in fair abundance), Chætoceras not until the latter part of May, and Rhizosolenia in early June. Rhizosolenia is usually as late as or later than this; Biddulphia is always one of the first forms to appear, sometimes causing a slight increase in the plankton as early as February, so it is really Chætoceras (some of the species of which are usually the most abundant of our April diatoms) that is this year unusually late.

I have just returned from Port Erin, where, during the first few days of this month, Rhizosolenia seemed to be unusually abundant. The water of the bay is visibly discoloured by it; when a tow-net at the end of a fifteen minutes' haul is raised from the sea it contains a column of thick soupy fluid, which runs out very slowly, as the meshes of the silk are largely clogged up with the diatoms; on a calm surface, with the sun shining, the peculiar iridescent glistening appearance characteristic of Rhizosolenia can be seen from the boat, and anything put into the water is at once covered by a slimy layer of the slender needles. It may be that Rhizosolenia has not even yet attained its climax, but a week ago the June increase in diatoms had certainly set in with unusual force. The "vernal maximum" has, then, this year been spread out and divided into three parts—a slighter increase in March (Biddulphia), a much greater one, later than usual, in May (Chætoceras), and another great increase (Rhizosolenia) early in June.

W. A. Herdman.

Liverpool, June 10.

On the Action of the Latex of *Euphorbia peplus* on a Photographic Plate.

In August, 1909, our attention was directed to certain properties of surgical importance possessed by the milky juice of *Euphorbia peplus*, a spurge naturalised in this colony. During the examination of the milky juice or latex, we exposed during two days in the dark a highly sensitive photographic plate some millimetres above a sheet of glass on which we had dried a few drops of the juice. On development, a sharp image of the dried juice appeared on the plate. We propose to describe briefly a few results from the many experiments since made to determine the conditions and nature of the action on the photographic plate.

Thirty drops of the juice were spread on a sheet of glass as the letters EUPHORBIA, and the glass dried in an oven at 100° C. for two hours. A sharply defined image of the letters was produced on development of a photographic plate (speed 325) placed 5 mm. above the dried juice after an exposure of forty-eight hours or longer. The density of the images increased with greater length of exposure, with thicker films of dried juice, and with less distance between the plate and the film. The edges of the images were well defined, as if focussed on the photographic plate. No image appeared with short exposures of twenty-four to forty-eight hours unless the films were very thick. With long exposures, up to thirty days, the images were more dense, but showed a little diffusion at the edges of the letters. When a distance of 15 mm. separated the photographic plate from the film, the plate was unaffected. Action on the plate diminished

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